

—Darwin, Pasteur, Helmholtz, Abel and Virchow for example, in Europe—Henry, Agassiz, Dana and Rowland in America—and also the encouragements and discouragements which are encountered by the men of to-day. Time will be required for the digestion of this material in order to discover the methods which are most efficacious in the advancement of knowledge.

Meanwhile, much co-operative counsel will be given by experts in various branches of learning. As soon as the general purposes of Mr. Carnegie's foundation were made known, hundreds of applications for assistance were received—the number of self-discovered "exceptional men" was large. The number of trivial applications for help in the prosecution of researches was surprising; but, on the other hand, the number of well-considered, important, fundamental inquiries suggested by men of the highest rank among the promoters of knowledge indicated that the entire income would all be absorbed at no distant day. Discrimination, therefore, became the paramount virtue—discrimination which should meet the approval and, if possible, the concurrence of the world's wisest men.

For this discrimination, the aid of specialists was indispensable. The astronomer was not the man to judge of biological claims, nor the chemist of economic problems. No board of "generals" could wisely act without the aid of a strong advisory staff of "adjutants." Accordingly, the authorities of the Carnegie Institution proceeded to select and enlist a number of advisory committees. Three, four, or five well-known authorities were chosen in each of the principal branches of science. All their expenses for travel and for clerical assistance were generously paid by the fund, but their services, like those of the trustees, were cheerfully given to the public without remuneration, and often at the sacrifice of time and convenience. Their hearty co-operation is a fresh illustration of the public spirit of men of science in our day, and their readiness to appreciate and help on the most deserving claims, irrespective of local or personal preference, augurs well for the efficiency of the Carnegie Fund and for the wisdom of the plans that will presently be adopted.

More specific announcements cannot be made until the trustees come together for their second meeting at the close of November next.

A careful perusal of Mr. Carnegie's language will bring out several points, to some of which I will venture to call attention. Here we have that special "endowment for research," which has been during the last thirty years and more the desire of so many men in England and America. This endowment is independent of any existing academy, university or school of technology; but it may co-operate with any that now exist or that may be established. It does not establish a university in Washington, which so many have advocated and so many have disapproved. Mr. Carnegie on this point is explicit and decided. The efficiency of the new institution is not restricted by any local, political or ecclesiastical fetters. Nor is there any attempt to decide what science includes. None of the progressive organised and systematic branches of knowledge are excluded. Economic, historical and archaeological inquiries may be aided as well as those which are more obvious to the public—physical, chemical, biological, geological and astronomical researches. Education may be encouraged, but it must be by the personal development of uncommon talents,—the advanced student, the young professor, "the exceptional man." To the last clause of his deed of trust, Mr. Carnegie attaches the highest importance. It corresponds with a clause in his gift to the Scotch universities. The trustees by a majority of two-thirds "may modify the conditions and regulations under which the funds may be dispensed"—if time, experience and changed conditions call for new arrangements.

NO. 1718 VOL. 66]

I cannot close this letter without reference to the great interest which this gift has aroused in all scientific circles at home and abroad. During the past summer, spent upon the Continent and in Great Britain, I have had the honour of talking with many men of eminence, everywhere known as investigators, and their counsel, suggestions and co-operation are not only an indication of the international character of science, but they give an assurance that the most enlightened experience of the world can be enlisted in the plans of this new foundation. At home, "it goes without saying," that there is the heartiest response to Mr. Carnegie's generosity.

With a grateful appreciation of the work of NATURE in the persistent advocacy of research. DANIEL C. GILMAN,
London, September 9. President of the Carnegie Institution.

Re Vegetable Electricity.

WITH reference to Dr. Waller's letter in NATURE, September 18, I confine my reply, in the limited space courteously offered me, to the main issue, *i.e.* the priority of research on the electric response of ordinary plants under mechanical stimulus. My footnote to my Linnean Society paper gave the published dates which must determine, as usual, such a question. It would only obscure the issue were I to take up here assertions resting solely on Dr. Waller's personal affirmation.

My statement which Dr. Waller wishes to traverse is definite enough, and may be answered in a definite manner. He has not done this. I stated that five months before the communication of his paper to the Physiological Society (November 9, 1901), Dr. Waller *heard* me describe my results on the electric response of ordinary plants under mechanical stimulus. My paper on the "Electric Response of Inorganic Substances: Preliminary Notice," was communicated to the Royal Society on May 7, 1901 (*i.e.* six months before Dr. Waller's communication to the Physiological Society). I read it before the Society on June 6. From the concluding portion of this paper I quote the short summary of the results obtained with plants.

"An interesting link between the response given by inorganic substances and the animal tissues is that given by plant tissues. By methods somewhat resembling that described above, I have obtained from plants a strong electric response to mechanical stimulus. The response is not confined to sensitive plants like *mimosa*, but is universally present. I have, for example, obtained such response from the roots, stems, and leaves of, amongst others, horse-chestnut, vine, white lily, rhubarb and horse-radish. The current of injury is, generally speaking, from the injured to the uninjured part. A negative variation is also produced. I obtained both the single electric twitches and tetanus. (Two response curves given to exhibit this.) Very interesting also are the effects of fatigue, of temperature, of stimulants and of poison. Definite areas killed by poison exhibit no response, whereas neighbouring unaffected portions show the normal response."

Dr. Waller not only heard me describe these results, but took part in the subsequent discussion of my paper. It is indeed very strange that he should on that occasion have said absolutely nothing about his being engaged in this particular investigation. An eminent physiologist declared during the discussion that the electric response of ordinary plants under mechanical stimulus was an impossibility. Dr. Waller, who immediately followed him, it is again remarkable to note, had not one word to say for the possibility of such a phenomenon! These facts are as significant as the fact that Dr. Waller communicated his paper five months after he had discussed mine at the Royal Society.

The above will dispose of the question of priority. My Linnean Society paper and Dr. Waller's paper read before the Physiological Society are now before the public. From these, anyone interested in the subject will be able to determine the scope of the two investigations, the novelty of the appliances and methods employed, and the accuracy of the results obtained.

JAGADIS CHUNDER BOSE.

THE claim for priority comes from Prof. Bose—implicitly by the note to his paper at the Linnean Society, to which I had to demur—explicitly in his present reply. Prof. Bose bases his claim on the final paragraph of a paper of June 6, 1901, now in

the Archives of the Royal Society. If this be regarded as a valid document and date of departure, I shall have something more to say about Prof. Bose's methods. If this date and document be not valid, his claim rests upon a paper at the Linnean Society of July 21, 1902, which seems to me to be a very interesting instance of scientific mimicry. Anyone interested in the study of such phenomena will find it instructive to compare the papers mentioned by Prof. Bose, of November 9, 1901, and July 21, 1902, to the Physiological and Linnean Societies respectively. I think he should also, as regards the general method, consult my Lectures on Animal Electricity of 1897 at the Royal Institution, which have been adopted by Prof. Bose as his point of departure.

A. D. WALLER.

British Association Meetings.

THE gradual decrease in the number of those attending the recent meetings of the British Association might suggest that the popularity or the usefulness of these scientific gatherings is on the wane. The opportunity for an instructive comparison exists in the fact that on the last three occasions on which the Association has met, it has repeated its visits to well-known centres, widely distributed. It might have been anticipated that, owing to the growth of material prosperity and of the population of these towns, a continually increasing number would have availed themselves of the advantages of these meetings. The following figures show, however, that the contrary is the case:—

Year.	Place of Meeting.	Number attending.	Year of previous Meeting.	Number attending.
1900 ...	Bradford ...	1915	1873 ...	1983
1901 ...	Glasgow ...	1912	1876 ...	2774
1902 ...	Belfast ...	1620	1874 ...	1951

Naturally the amount of grants for scientific purposes shows a similar decline:—

Bradford, 1072 <i>l.</i>	against 1685 <i>l.</i>	in 1873
Glasgow, 945 <i>l.</i>	„ 1092 <i>l.</i>	„ 1876
Belfast, 960 <i>l.</i>	„ 1151 <i>l.</i>	„ 1874

The usefulness of the Association in one direction is apparently lessened, since it has distributed about 1000*l.* less in the three years, but it may be that there is not the same necessity for assistance as was the case a quarter of a century ago, and that consequently the amount applied for by the different sections has not been as large as on previous occasions. But this does not put aside the fact that there is a distinct falling off in the interest exhibited, as tested by the numbers attending.

Supposing there is any decrease in the popular favour, and the smaller figures are not due to temporary causes, it seems worth while to ask whether any portion of the decline is traceable to reasons connected with the Association itself. This is a question which can be answered only by those who are intimately connected with the management, but there was a feeling among some of the members that the business was unduly protracted, and it was asked, with some apparent show of reason, why the meeting must always begin on a Wednesday. If the President's address, it was urged, was given on Monday evening, it would allow four clear, uninterrupted days for the business of the sections, which in most cases would be found sufficient, and then the Saturday could be employed in the manner it now is, or in winding up the unfinished sections. There may, of course, be an insurmountable objection to altering the arrangements which have existed for so many years, but which scarcely seem to meet the conditions of modern life, and it is with the view of hearing from some authoritative source the object of maintaining the old order of things that I have ventured to trouble you with this note.

W. E. P.

September 19.

Helmholtz on the Value of the Study of Philosophy.

THE opinions of Helmholtz, even as expressed in his popular scientific lectures, have such permanent weight that you may consider the following correction of sufficient general interest to publish it in your journal.

On p. 234 of Dr. Atkinson's "Popular Lectures on Scientific Subjects by H. von Helmholtz" (second series, new edition, Longmans, Green and Co., 1893), lines 7 to 11, we read:—

"And the physician, the statesman, the jurist, the clergyman, and the teacher, ought to be able to build upon a know-

ledge of *physical* processes if they wish to acquire a true scientific basis for their practical activity." (The italics are mine.)

What may have been Helmholtz's opinion of the value of a knowledge of *physical* science to the groups of specialists above named may be gathered from other parts of his writings, but in view of the surely unjust discredit into which the study of genuine philosophy (such as Helmholtz defines it) appears to have fallen in the eyes of the followers of the "Naturwissenschaften," it would appear just to quote the original passage, whereby it will be seen that what was perhaps a printer's error in the translation has altered the whole gist of the passage:—

"Und auf die Kenntniss der Gesetze der *psychischen* Vorgänge müsste der Arzt, der Staatsmann, der Jurist, der Geistliche und Lehrer bauen können, wenn sie eine wahrhaft wissenschaftliche Begründung ihrer praktischen Thätigkeit gewinnen wollten" (Helmholtz, "Vorträge und Reden," p. 189, fourth edition, second vol., Braunschweig, 1896). (The italics are mine.) That "*psychischen*" is *not* a printer's error for "*physischen*" in the original is evidenced by the context, which is so interesting that I venture to quote it. After a brief comparison of the relation of philosophy to metaphysics with that of astronomy to astrology, Helmholtz says:—

"Ebenso bleibt der Philosophie, wenn sie die Metaphysik aufgibt, noch ein grosses und wichtiges Feld, die Kenntniss der Geistigen und seelischen Vorgänge und deren Gesetze. Wie der Anatom, wenn er an die Grenzen des mikroskopischen Sehvermögens kommt, sich Einsicht in die Wirkung seines optischen Instrumentes zu verschaffen suchen muss, so wird jeder wissenschaftliche Forscher auch das Hauptinstrument, mit dem er arbeitet, das menschliche Denken, nach seiner Leistungsfähigkeit genau studieren müssen. Zeugnis für die Schädlichkeit irrtümlicher Ansichten in dieser Beziehung ist unter Anderem das zweitausendjährige Herumtappen der medicinischen Schulen."

I have not access to earlier editions of the original German than 1896; relatively to my object, such reference seems unnecessary.

B. BRANFORD.

The Technical College, Sunderland, September 23.

Trade Statistics.

DR. MOLLWO PERKIN repeats in NATURE, p. 443, Mr. Levinstein's statement that in foreign trade "we went back during the ten years 1891-1900" (*Journ. Soc. Chemical Industry*, pp. 893-4). The evidence given is that "in the year 1890 our total exports amounted to 328 millions sterling," whereas "the average amount during the decade 1891-1900 was only 300 millions." But why should 1890 be taken as the standard year? It happens that the exports in that year were unusually high—higher, indeed, than in any other year from 1880 to 1898. Had Mr. Levinstein been in a hopeful frame of mind, he might have chosen 1888 or 1892 as his normal year, or, much more rationally, he might have taken the average of five years, 1886-1890 (299 millions), or the average of ten years, 1881-1890 (297 millions). Any of these methods would have brought out the more pleasing conclusion that our foreign trade is advancing. My object is not to decide whether it is or not, but to protest against Mr. Levinstein's method of proof. Can we imagine a meteorologist contrasting the average rainfall of a series of years with the rainfall of a *single* preceding year and on that basis announcing a change in the climate?

The facts (often exaggerated and misunderstood) as to the more rapid advance of German exports are fully and clearly stated in "Comparative Statistics of Population, Industry and Commerce," recently issued by the Board of Trade at the price of 5*d.* It is not clear why Mr. Levinstein makes use of the British "total" exports, including all the transit trade, while for Germany he takes the "special" exports, from which the transit trade is, as far as possible, excluded. This swells all the British amounts by something like 25 per cent. beyond what they would stand at if they represented native produce only. It does not, however, much affect comparisons of rates of progress. But it confuses abstractors—in Dr. Perkin's abstract the distinction is overlooked.

F. EVERSHERD.

Kenley, Surrey, September 9.

It is quite true, as Mr. Eversherd points out, that the exports for 1890 were unusually high, but those of 1899 and 1900 were also exceptional, owing largely to war exports; this, however, hardly alters Mr. Levinstein's contention—that the trade of the country shows a decline as compared to the trade of